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TO : All holders of Vol. 19, No. 2, STUDIES IN INTELLIGENCE, Summer 1975

The review of Philip Agee's book, Inside the Company: CIA Diary, which appears on pp. 35-38 of the referenced publication, should be classified SECRET, rather than unclassified.

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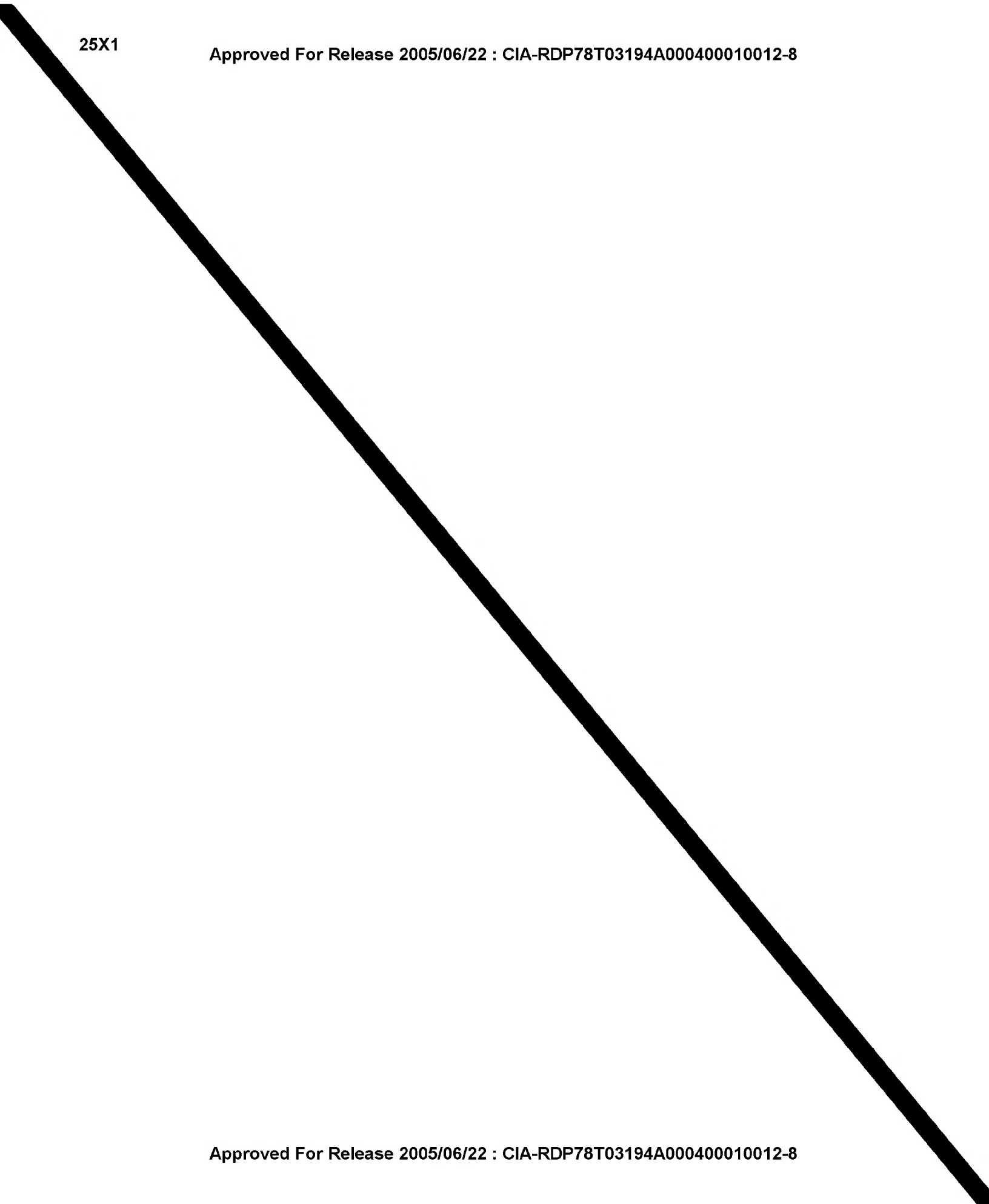

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The following article is the summary of a detailed study prepared for the Center for the Study of Intelligence of the Office of Training on the recurrent topic of the intelligence dilemmas arising from security requirements within the framework of a free society. We hope this statement of the problem will stimulate further thoughts on the subject.

The Editor

SECRECY AND INTELLIGENCE IN A FREE SOCIETY

James E. Knott

In discussing what I believe to be the major areas of concern that our free society has evinced regarding secrecy and intelligence, I hope to make it clear that I feel there are no final answers. They are not problems that can be solved; they are focal points that will demand continuing attention in pursuit of a balance which must be worked out between the opposing factors.

The central problem which demands attention does not stem from the question whether secrecy, intelligence, or even clandestine operations are compatible with a free society. The central problem is the structure through which that free society oversees its processes of secrecy determination, intelligence production, and the conduct of clandestine operations.

This may appear to be a mechanistic conclusion, but I make it because I am convinced that our free society is in basic agreement as to the kinds of things on which secrecy is justified. I am also convinced that—if the society knew more about the subject—there would be a consensus on the criteria which should be applied to deciding whether or not a foreign clandestine operation was an appropriate activity for a free society. And, in complement to such agreement, there is the fact that the virtue and blessing of a free society is that there is a constant and continuing process which defines and refines the values the society expects to be applied by its institutions. These values themselves do not change radically—but neither are they absolute. They adjust to the efforts the society is called upon to undertake, and they adjust in particular in accordance with the threats the society feels it faces. In other words, the free society will relinquish some of its freedom if that is necessary, but it will wish to see readjustment take place once such relinquishment is no longer necessary.

The inherent feature of secrecy is the limitation of access to the secrets. The free society as a whole cannot make the judgment as to whether or not individual matters are legitimately kept secret. It must place its trust in an oversight body or bodies to act in its behalf. The smaller the number of people it decides it needs to establish such a condition of trust, the better it will be for the secrecy system.

The free society must have confidence that its oversight mechanisms have adequate access to secret material to make judgments, and that this judgmental process is being exercised independently. There has to be trust that secrecy is not being used against the best interests of the free society; that the activities which are being protected by secrecy are being conducted effectively; and that necessary

readjustment of these activities takes place in conformance with changed domestic and international circumstances. It is this confidence and this trust in the oversight mechanisms which has broken down.

In exploring the means by which confidence and trust can be restored, the free society must bear in mind the fact that its consensus does change. The lessons of the past must not be ignored, but it would be an error to judge what was formerly done—or what might be done in the future—by a consensus of the current moment deprived of historical perspective. It would also be mistaken to concentrate too much on preventing the abuse of secrecy without also recognizing that there are legitimate secrets. The free society owes it to those it holds responsible for producing secret information and conducting secret activities to maintain an oversight process which protects legitimate secrecy.

What then are some of the suggestions for improvement which should be considered? I have grouped them under five headings:

Redefinition of Government Secrecy

"National security" alone is an inadequate base for a government secrecy classification system. Some suggest expanding this to "national defense or foreign policy." Executive Order 11652 uses "national defense or foreign relations" and then combines the two into "national security." However, as I have noted, the Freedom of Information Act not only excludes from its procedures those national defense or foreign policy secrets which have been "properly classified," but also excludes eight other areas, such as trade secrets and certain investigatory records. Such matters are not part of the classification system, but one suspects that a good many of them get mixed up in the classification system of those agencies dealing with national defense and foreign relations secrets.

If it could be granted that there is overall confusion about governmental secrecy in our free society, wouldn't it be better to have a comprehensive system? Or would formalizing what already exists in practice only compound the already overwhelming problems of dealing with government paper? Nicholas deB. Katzenbach, discussing this only in the foreign policy field, comes down in favor of major surgery on the classification system and relying "on the good sense of bureaucrats to keep confidential what should be confidential most of the time, without employing bloated concepts of national security to do so."* Perhaps so, but I believe the opposite course of inclusiveness is worth exploration.

In any case, whether the lesser secrets are dropped out of the currently overblown "national security"-based classification system into a system of government-wide applicability, or whether they are dropped to the level of reliance "on the good sense of bureaucrats," there can be no doubt of the need for drastic reduction in what has formerly been placed in the national security category. What is needed is much greater clarity as to what this category should *really* contain. Better guidelines would help immensely in the judgmental factor which will always be involved. At the same time, the numbers of persons entitled to make such judgments must continue to be reduced. Some such clarifications and further reductions, it seems to me, will be the inevitable results of current attempts to cope with the major changes brought about by the Freedom of Information Act and Executive Order 11652.

*Katzenbach, "Foreign Policy, Public Opinion, and Secrecy," *Foreign Affairs* (Oct., 1973) p. 17.

Another area that needs clarification has to do with abuse of the classification system. On the one side, it has been much too easy to overclassify. A Subcommittee headed by Congressman William S. Moorhead conducted a study in 1971 that found there had been 2,433 investigations by government agencies of classification system violations over a four-year period. Of these, only 2 involved cases of overclassification and "not a single administrative penalty was imposed against overclassification."* On the other hand, great concern has been expressed about dangerous leakage in the system—"unauthorized disclosure." No one would deny that there are legitimate secrets which deserve greater protection. Clearly the current Espionage Act is inadequate for this purpose. One doubts, however, that it will be improved upon until secrecy has been reduced to the level the national consensus will feel is justified and our free society becomes more convinced than it is at present that there are adequate intra-executive means of airing and reconciling legitimate dissent.

Congressional Oversight

It is, of course, up to the Congress as to how it organizes its oversight role. The current system has come under a great deal of attack, notably from members of Congress itself. At least some modification, and possibly even major change, in the four-subcommittee system appears to be in the offing. Whatever means of rebuilding trust and confidence are found, there is one primary fact of life about secrets which must be faced: those who have been made responsible for secrets they feel are important cannot be expected to continue a system which endangers the secrets. There must be trust and confidence on both sides of a secrecy-sharing process. In a free society, the official who feels secrecy has been and will be violated cannot have and should not have the option of evasion of legislative oversight. His only option is to point out the consequences of poor security and the fact that the activity must cease if the secrecy necessary to its continuance cannot be preserved. And, does anyone deny that the publicity-attracting nature of clandestine operations creates special problems in establishing mutual trust and confidence?

Another matter to be considered with regard to oversight are the interests of the men concerned. The primary role of the intelligence community will undoubtedly remain one dealing with military security matters. However, other fields have been increasingly added, notably international economics, narcotics intelligence, and international terrorism. Further, there is a special need to view the intelligence community as a whole, and the members of that community relate to quite a variety of authorization committees. There needs to be a means of promoting greater Congressional cohesion between these differing jurisdictions.

Other than including people who have the trust of their Congressional colleagues, whose composition unifies the field of intelligence yet reflects its diversified content, who can follow methods preserving secrecy, there is the key question of how much detail the oversight body needs. British intelligence authority John Bruce Lockhart's central thesis on this question is: "the operations of Secret Services must remain secret, but the principles by which Secret Services can best be directed and controlled should be considered carefully, discussed, and understood by those at government level who are responsible for controlling Secret Services."** Not having a

*Rep. W. S. Moorhead, "Operation and Reform of the Classification System in the U.S.," in Frank and Weisband, ed., *Secrecy and Foreign Policy* (Oxford University Press, New York, 1974) p. 101.

**John Bruce Lockhart "The Relationship between Secret Services and Government in a Modern State," *RUSI, Journal of the Royal United Services Institute for Defense Studies,* (June 1974) p. 3.

parliamentary system, we in the United States need to have such consideration, discussion, and understanding shared by the executive and legislative bodies. It is extremely important to note that what Lockhart urges be left out of the discussion are the details of the "*operations of Secret Services.*" The application of such a concept to CIA is not as radical as it might appear, inasmuch as only a portion of what CIA does is made up of the "Secret Service" kind of operation—and much that is supposed to pass as clandestine, really isn't.

Perhaps such exclusion of clandestine operations from examination may not be found satisfactory, however. Sometimes detail is needed for making evaluations. Sometimes knowledge of specifics is needed to be able to ask the right general questions. Does examination of detail need to be seen as an ongoing process, or might it be seen as temporary—until confidence was restored? Would examination of detail need to be across the board, or could the need be met by periodic or spot checks? Could detail be restricted to one type of operation, and the others left alone?

Lastly, when an examination or follow-up probe involves very sensitive material, does the full committee (or committees) need to be a part of such an examination? Couldn't one or two members, possibly on a rotating basis, be assigned to the task? Or, preferably, could such a question be transferred to some such body as the President's Foreign Intelligence Advisory Board, which would then have the responsibility of standing behind a reassurance of the oversight group. Or, could such inquiry be undertaken by a very small number of particularly trusted and reliable Congressional staffers? And what open record is at all possible on such matters to help reassure the free society and improve acceptance of appropriate joint responsibility? Could, for instance, some sort of quarterly listing of general topics covered by oversight proceedings be made public?

Executive Oversight

Executive oversight is not as critical a matter at the moment as legislative oversight, but it too merits attention. The primary concern of our free society at this time does not seem to be whether or not the Executive knows what CIA does, but whether the Executive will be able to abuse the secret capabilities represented by CIA. The meeting of the problem of legislative oversight and the functioning of a much more open Presidency should result in overcoming this fear.

This does not mean that there should be a return to the secrecy which used to surround the clearance procedures for CIA activity. The channels for executive approval of CIA activities should be uniform and not competitive or duplicatory, so that no future charges of CIA selecting the most favorable channel can be made. The channels should be publicly known, and so should the people in them. Again, it should be as much a matter of principles rather than details on operations whenever possible, but obviously when details are required in order to make risk/gain assessments, they must be readily provided. Clearly, such details will be required very often. Full knowledge can sometimes provide a better base for cooperation on the preservation of secrets than a partial knowledge leading to shared speculation between those partly "in the know." How often an operational activity needs to be reviewed, and the number of people who need to give their approval, can depend on the type of operation involved.

The "grey" area between CIA's domestically-based but foreign-related activities and those of the FBI must be reduced to an absolute minimum. There must be clearly

understood procedures for an accountable ruling in case of any doubt. Domestic activities must be governed by the standards and institutional arrangements of the domestic scene, and it must be clear to the free society that this is the case. There must be a very minimum of overlap between the decision-making process for domestic activity and the decision-making process for foreign activity. The two must be judged by different standards.

Lastly, there is the problem of efficiency and effectiveness. There is a great deal more of the administrative side of the intelligence organizations which could be open to Congressional scrutiny. However, the major responsibility obviously rests with the executive branch, which must continually improve its management practices. More rigorous, not less rigorous, review by the Office of Management and Budget is needed. Continued progress must be made on the community-wide framework of requirements against which evaluations can be made. The techniques of evaluating programs must also be improved. There must be evaluation in depth on a selective basis—a requirement, a source, a station, etc.

Reduction of Agency Secrecy

Without the shadow of a doubt, a sort of Gresham's Law operates with regard to respect for security systems. If an employee is asked to treat worthless material with the respect due only to worthwhile secrets, the bad practices will drive the good practices out of circulation. Similarly, if a free society is asked to respect a security system and then finds that the system has protected "bad" or worthless secrets, it may well result in damage to the system's ability to protect "good" secrets. From both the standpoint of the employee's observance of the security structures and the free society's respect for maintaining security systems, there can be only one conclusion: the matters which need to be kept secret must be reduced to a minimum.

For a conclusion so obviously correct for a free society, it is hard to see why there should be any disagreement or serious problems. But it is vastly easier to state such a conclusion than it is to implement it. It seems to me that the problems of implementing it for the Agency stem from three main sources. The first of these is an insufficient differentiation between the security needs of the varied personnel of CIA. To draw again on the wisdom in this field which John Bruce Lockhart has set forth:

> Those in control of Secret Services must have a realistic and disenchanted understanding of "security." This is not as simple as it sounds, because possibly more follies have been committed in the name of security than in any other governmental activity in a modern state. These broad principles must continually be borne in mind if this area of folly is to be reduced.
>
> In secret operations there are only two degrees of security. One is the suit of armour, where the man's identity or objective remains a total secret. The other is the fig leaf, where a facade of respectability is imposed on functions or individuals whose real purpose is widely known and accepted. Security trouble arises when it is believed by those who control them that there are degrees of security in secret operations between the suit of armour and the fig leaf.*

Those who are *really* operating in secret need the "suit of armor" and need every help in keeping it impervious. Those who are operating under "fig leaf" conditions should not be treated the same way as those within armor. It should also be fairly

Ibid, p. 5.

unlikely that the "fig leaf" operator would revert to or become a truly clandestine operator. A great many of the Clandestine Service personnel now have the trappings which are the due of the "suit of armor" operators but they are in fact engaged in "fig leaf" operations. The easily identified large-scale operations of Vietnam, Laos, and Cambodia come most easily to mind. However, this is also true of many of the liaison arrangements with foreign intelligence services. It may also involve such new missions as anti-narcotics and anti-terrorism activities conducted in cooperation with local authorities.

Such "fig leaf" operations may well be fulfilling agreed and necessary functions; they may well require some clandestine skills; and in some cases they may well be dangerous. But they do not require the high degree of protection of identity, skills and movements necessary for the truly clandestine operator. Add to this need to differentiate between operators requiring "suit of armor" protection and operators who need only fig leaves, the further differentiation between operators and the rest of the CIA personnel. Do people who are only handling secrets even need a fig leaf?

This area of difficulty can be compounded by the "one Agency" concept—the idea of interchangeability of Agency careers. In my personal opinion, this is a mistake in so far as it presumes a movement from the analytical side into the Clandestine Service. It has been done, but how often? And how many of those who did make such a transfer actually become clandestine operators?

Possibly the greatest source of difficulty on this differentiation problem could be the extent to which there may be an effort to hide the operators within the larger group of Agency employees. According to Roger Hilsman: "the original idea of CIA had been to conceal the cloak and dagger activities behind the much larger mass of 'overt' intelligence work—research and estimating, monitoring foreign propaganda broadcasts, and so on."* I do not personally know if this was indeed the intent. To the extent that it may be, such "cover" should be questioned as to its usefulness. At best it far more resembles a fig leaf than it does a suit of armor. And society would really not need to blush if this particular fig leaf were dropped.

In sum, the "one Agency" concept deserves a very hard look in terms of its consequences for personnel security practices. And the degree to which the personnel security practices of the Clandestine Service are based upon "suit of armor" assumptions also needs close examination. Are the justified needs of truly clandestine operations being endangered by being too widely applied? Shouldn't the truly clandestine be set apart as urged by another of Lockhart's principles: that the "operational front of secret operations should be as narrow as possible?"**

The second main source of problems in reducing security practices to a minimum are what must be regarded as national bureaucratic tendencies inherent in any organization, but particularly large ones. Bringing about some uniformity in judgmental matters is extraordinarily difficult and in practice the "lowest common denominator" is subject to continual decline—particularly if there is no penalty for "playing it safe." Such penalties should be set up *and used*. There is no final answer, of course, but some clearer criteria need to be set up and there must be an improvement in systems of review—an excellent function to assign to deputy chiefs.

Another major factor to be attacked are practices stemming from tradition and precedent. Such practices do not necessarily represent accumulated wisdom.

*Hilsman, *To Move a Nation*, (Doubleday, New York, 1967) p. 79.

**Lockhart, *Op. cit.*, p. 5.

Sometimes they do, but they can also represent outmoded ways of doing things which historical circumstances may have once justified—circumstances which subsequently departed the scene. There are, for instance, "worst case" regulations. These were set up when a "worst case" did occur or when someone had the imagination to think that it might. Such "worst case" regulations need to be examined to see what the probability really is of such an event occurring. All too often such regulations stay on the books, are not enforced by the authorities, but are available as a basis for supervisory thunder "just in case." This is dishonest administration, natural as it may be. Another group of practices undoubtedly stem from a "weakest-link" concept. At some particular point a given security practice may well have been set up or reinforced to prevent it from being the "weak-link" in a chain of security practices. Its chain may no longer exist, or other parts of the chain may have become of a much weaker gauge. It is absolutely right to view security practices in a systems approach flow context. But differentiated flow channels are possible and can be treated differently so that what would be a "weak link" in one wouldn't necessarily be so in another.

Besides being looked at in a systems approach chain method, security practices should be examined as a layered concept. Is the secret at the core still a secret? Are the various layers of protection ("derivative" secrets) still needed or can some of them be relaxed or dispensed with? How many practices may have come from the requirements of some other body as part of the process of establishing the mutual trust needed for the exchange of secrets? Are these still needed?

A third main source of problems is the necessity of not disclosing too many clues as to your intelligence successes—or lack thereof. This is what is involved in the reluctance to disclose too much information about Agency organization or budgeting. It is held that such information could show trends which ought to be concealed. One suspects that some such trends would be fully evident from open policy documents, i.e., increased concentration on the Mid-East, decreased attention to Indochina, increased interest in economic information, etc. Further, even in the open parts of our system, it is often very difficult to track expenditures from budget year to budget year. Without denying that some trends merit concealment, one can't help wondering in how much of the agency this may be a problem, and at what level of budgetary listing it becomes a problem. Much information is justifiably withheld because it meets the statutory protection provided in the 1949 Act for intelligence sources and methods. But isn't there a good deal of such *organizational information* which would not endanger sources and methods?

Turning from organizational information, what about making more of the intelligence end-product available to Congress and the public? If this can be done without endangering sources and methods, or endangering what I regard as legitimate executive leadership rights and administrative responsibilities, I feel much more such information in an appropriately usable form should be made available. Such sharing is indeed on the increase. The more that it is possible to do this with central intelligence, the less possible parochial manipulation through partial release of information becomes. It has been suggested that the Congress should be able to levy its own estimate requirements on CIA, and this is an idea worth exploring.

Procedures for promoting change

The discipline of the marketplace brings change. Much of what CIA does cannot be out in the marketplace. Being responsive to a need to change and adjust poses very

special problems for a closed organization. There is a need not only for CIA to be much more closely attuned to the consensus of our free society, but also for a reinforcement of its processes of eliminating the mediocre and the outdated.

CIA has had procedures to promote change, but I believe it is fair to say that they did not work well enough. Undoubtedly a part of the reason for resistance to change stems from a humanitarian concern about men whose services might no longer be required. Another part might stem from a cautious reaction to preventing an over-use of the Agency such as had marked certain periods in the past—an over-use which can produce failures not balanced in the public mind with successes. It might have derived from a realization that it would be much more difficult to operate in a multi-polar world where the choices were less clear and where the cement of common assumptions characterizing the Cold War period would be lacking. It may well be prudent in some cases to keep standby capabilities until you are more certain that you won't need them. However, much necessary change didn't take place simply because it didn't have to.

Beyond the need to reinforce external procedures of promoting change, there is a need to examine CIA's internal methods serving this purpose. Where did recommended change take place and where did it fail to take place? What was the record as regards Inspector General surveys? Where was lip service paid to their recommendations but little actually ended up being changed? There were processes of feedback and some attempts at evaluation. What happened to these? What is the record on Management Advisory Groups? What was the upshot of training programs designed to help challenge assumptions and promote rethinking? There should be a considerable body of material available for analysis on what must be one of the key problems of secrecy and intelligence in a free society.

To conclude: free society needs intelligence. It needs secrecy. But there has been a loss of proportion, a loss of confidence and trust, and a lack of understanding on all sides. These must be overcome because the free society needs to make wise use of the capabilities at its command—and I include covert capabilities in this. It is high time that a mending took place.

On bringing all of the analytical disciplines to bear

THE CASE FOR A HOLISTIC INTELLIGENCE

Lloyd F. Jordan

The central thesis of this paper is that the increasing complexity of national security problems requires that the Central Intelligence Agency adopt a new approach to intelligence analysis. This approach requires that intelligence problems which have important political, economic, scientific, military, and other salient dimensions be treated in a manner that will assure from the outset that the interplay of these various factors is taken fully into account. Since this thesis is based upon a belief that the separate treatment of each of these factors is inadequate because the problem as a whole is more than just the sum of its parts, it can be referred to as a *holistic* approach. The finished intelligence product of such an approach would be qualitatively more than the mere sum of its parts by virtue of an extra dimension provided by their integration at every stage of research—from the development of the research design to the completion of the analysis.

The following discussion is focused on: (1) the two dominant characteristics of the Agency's analytical process which make it deficient in meeting today's national security requirements; (2) the rationale for and explanation of the proposed new approach; and (3) the organizational and management implications of adopting such an approach.

It is not the purpose of this paper to present a summary indictment of the past and present modes of intelligence analysis; it is rather an attempt to identify the reasons for their inadequacy and to define a new analytical approach which will enable the Agency to cope better with the increasing number of complex intelligence questions confronting it. Whatever is critical of past and present approaches to intelligence analysis is intended as constructive criticism.

Dominant Characteristics of the Analytical Process

The approach to intelligence analysis within CIA has two dominant characteristics which impair the Agency's capability to deal most effectively with complex intelligence problems. First, despite the fact that the political, economic, scientific, and military aspects of intelligence problems have become increasingly interwoven, intelligence analysis tends to treat each of these dimensions independently of the others. Political intelligence is produced as a final product primarily by political scientists and historians, economic intelligence by economists, and scientific and weapons intelligence by physical scientists and engineers. Second, intelligence analysis has been and continues to be carried out largely without the consideration of additional aspects of intelligence problems which all now agree are important. For example, many of our major intelligence problems need to be analyzed from the perspectives of sociology, social psychology, and cultural anthropology as well as from more traditional viewpoints. The negative impact of these two characteristics of Agency analysis can best be discussed in the context of the phases of the Agency's analytical process.

Intelligence analysis within the Agency can be characterized as a three-stage process: (1) "building block" research, (2) intermediate-level analysis, and (3) synthesis, or the production of national intelligence estimates.

The "building block" phase of research involves the accumulation, sorting, and organization of the vast amounts of information received which pertain to matters of intelligence concern. It produces the underlying studies that constitute the basis for subsequent, broader-gauged analysis intended to answer specific intelligence questions. Because its focus is on the organization of informational fragments, the "building block" phase of research lends itself—indeed, requires—a microscopic approach taken from the point of view of individual aspects or disciplines if it is to be done efficiently and with sophistication. Such research is an indispensable continuing intelligence function. The need to perform it, however, differs in degree in various problem fields and geographical regions. Much of the crucial information needed for analysis is frequently unavailable to the intelligence analyst. Therefore, the sifting, weighing, piecing, and structuring of bits and fragments of available information on a particular problem is indispensable if the analyst is eventually to have any foundation upon which to make intermediate-level analysis and intelligence estimates.[1]

CIA's work over the years in developing "building block" analyses on a country and problem basis has been and remains impressive. The work, for example, begun in the 1950s and extending into the early 1960s on the Soviet and East European economies, political systems, scientific and technical efforts, and weaponry development attest to this excellent performance. More specifically, the numerous research aids produced in the Office of Scientific Intelligence (OSI) in the 1950s on the Soviet Academy of Sciences, its departments, their structure, staff, and research plans, the status of various fields of Soviet science and engineering were indispensable first steps in the structuring of a meaningful data base upon which later more sophisticated assessments of Soviet achievements and prospects for development in various scientific fields and in weaponry were made. Likewise, the pioneering "building block" analyses of the quantity, types of specializations, and quality of Soviet and East European scientific and technical manpower were carried out in OSI through a number of highly specialized studies. Similarly, the research in the 1950s and early 1960s on the Soviet Bloc economies provided the foundations for later more sophisticated economic intelligence analyses. There is, however, somewhat less need now for such work in many problem areas of the USSR and East European countries because both raw data and finished intelligence have been built up to substantial levels, though undoubtedly new problems will continue to arise demanding that such basic research be undertaken. In contrast, "building block" research will continue to be indispensable to intelligence analysis on Communist China across a broad spectrum of problems for several years.

On the intermediate level of analysis, the objective is to aggregate and synthesize the material developed in various "building block" studies to produce interpretative and predictive intelligence analyses. The monodisciplinary microscopic approach that is so important for "building block" research has had, and continues to have, an unfavorable influence upon the analysis work at the intermediate level in two major respects. First, multidimensional problems are approached too narrowly; i.e., they are not considered from all relevant aspects. Second, too little attention has been given to spelling out exactly how the analysis undertaken will lead to the answers sought and how underlying assumptions or uncertainties must qualify the results. A review of prefaces and introductions to

[1] It is in this "building block" phase of research that the analysts in the Central Reference Service frequently make crucially important but frequently unheralded contributions.

intermediate-level intelligence analyses—where the writer really owes the reader an explanation of what it is he is about to do—of a number of different intelligence problems reveals that studies at this level almost exclusively attack their problems from a single point of view and without detailing the conceptual basis upon which the analysis will proceed.

The National Intelligence Estimate (NIE) process prior to 1973 was designed to develop a synthesis of separate finished intelligence studies on a variety of problems to support U.S. national security policy making. A review of the NIEs, however, reveals that they, too, reflected the monodisciplinary approach to problems present in the intermediate-level analyses prepared in the intelligence production offices. This is not to say that a given NIE, for example, on Soviet military research and development and others did not frequently incorporate sections on budgets, S&T capabilities, quality of manpower, etc. The fact is, however, that these particular analyses were, in large part, produced in different offices by several distinct organizations focused upon separate specific pieces of the problem. It was really only at the National Estimates level of analysis that an effort was made to put all the pieces together into some meaningful whole.[2] Too often it was done by giving serial consideration to each of the distinct aspects of the problem. The Office of National Estimates (ONE), furthermore, was traditionally the preserve of the historians and political scientists, with only an occasional infusion of expertise in other disciplines in the latter years of its existence.[3] Beyond the resolution or accommodation of Agency differences, the synthesis that occurred at the ONE level was essentially that of a style and format and to a lesser extent substantive.[4] ONE's failure to deal in a satisfactory manner with the interactions of various aspects of the problems it faced cannot legitimately be attributed exclusively to it as an organization *per se* as much as to the type of analytical inputs it received. It is virtually impossible to integrate meaningful discrete pieces of analysis on different but related facets of a complex intelligence problem after the research on the various pieces has been completed by analysts using different assumptions and sometimes mutually exclusive analytical approaches.

A number of CIA intelligence officers involved in analysis have recognized in recent years the need to mount a different type of attack on intelligence problems. A common concern is expressed in their writings about the need for the improved integration of intelligence analyses relevant to particular multidimensional problems.[5] They presented good evidence that the analytical process left much to be desired in this respect. In his dialogue with Mr. Shryock on the issue of bringing various schools of thought in Sovietology to bear on intelligence analysis on the Soviet Union, Mr. Whitman stated that:

> The national estimating process contributes even less to the synthesis of methods and insights for which Mr. Shryock calls.
>
> While the drafters of an NIE may be partial to one or another of Mr. Shryock's schools, they perform little sustained research on their own and are in

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[2]Ludwell Montague, *Studies in Intelligence, XVI/2.*

[3]Individuals with engineering and scientific backgrounds assigned for limited periods to ONE were John Kerlin, Jim Porter, and Herb Orlins; with economic backgrounds, Edward Proctor, Louis Marengo, and Penelope Thundberg, and possibly a few others.

[4]John Whitman, "Better an Office of Sovietology," *Studies in Intelligence, VIII/1.*

[5]Richard W. Shryock, "For an Eclectic Sovietology," *Studies in Intelligence, VIII/1.* John Whitman, *op. cit.*

principle eclectic. Their estimate is produced with little participation by the multifarious units of Sovietologists tucked away in various parts of the community.[6]

In general, however, these analysts sought mechanisms for introducing additional viewpoints into the synthesis phase of the analytical process, rather than urging changes that would ensure that all relevant aspects be taken into account in the earlier phases of that process.

The present National Intelligence Officers (NIO) structure has potential for creating an environment that could be conducive to the implementation of holistic approach to intelligence problems on a geographical or functional basis. Several of the Key Intelligence Questions (KIQ) strategies implicitly suggest the need for taking into account the multifarious aspects of the intelligence problems with which they deal. Inter-office projects or joint studies are mentioned in these papers; they reflect an effort to synchronize analyses on the various dimensions of a given KIQ. This approach, however, falls short of providing the type of integrated analytical focus to be advocated here because, once again, the interactions between the various aspects of the problem are left to the NIO to recreate late in the game on the basis of separately prepared inputs.

In addition to the virtual absence over the years of any real integration of all problem aspects in intelligence analysis, there has been virtually no attention given to the perspectives of other disciplines such as sociology, social psychology, and cultural anthropology. It appears that the policy for staffing the analytical components of the Agency over the years has omitted the hiring of analysts with training or experience in these three disciplines. This is not to contend that a number of people with such backgrounds have not been employed by the Agency in various capacities, but it appears that they were not recruited for the specific purpose of performing intelligence analysis from the perspectives of their disciplines. The pattern of staffing, therefore, has restricted significantly the spectrum of disciplines used in the solution of intelligence problems.

The following case is illustrative. Since the inception of SALT, considerable interest has been expressed by both analysts and policy makers in Soviet perceptions of U.S. policies and intentions. The perception problem has also been raised in the context of the relations among China, Japan, and the USSR. Despite major contributions to the field of elite perception analysis—mostly by social psychologists and political scientists—they have been largely neglected in the intelligence analysis community. For intelligence purposes, there is a need to assess what has been done in the academic community and to determine if and how such research can be adapted to intelligence analysis.[7] Our past failure to incorporate such work has constrained the Agency's ability to deal with some of its most important current problems.

The Case for a New Approach

To remedy these two deficiencies in the Agency's intelligence analysis sector, it is necessary to adopt a *holistic* research approach to intelligence analysis at the intermediate and estimative levels. This higher plateau of analysis must rest upon the foundations of polydisciplinary research combined with monodisciplinary studies at the building-block phase, undertaken from disciplinary perspectives heretofore largely

[6] *Ibid.*, p. 65.

[7] The Office of Political Research recently initiated a literature assessment on approaches to perception analysis in the Analytical Support Center.

untried in the CIA.[8] This approach can only be achieved by assigning analysts to a given intelligence problem with disciplinary expertise relevant to its various facets in a multidisciplinary or interdisciplinary research mode. More specifically, such an approach would require that groups of analysts jointly embodying the capabilities required to deal professionally with all the significant aspects of an intelligence problem would work together as a team toward its solution. Communication between team members and their mutual approach to the problem would have to be such that a full understanding of the interactions between its economic, political, technical, strategic, cultural, and sociological factors can be understood and delineated in a form suitable to guide their analysis. So elaborate an approach to *all* tasks is obviously not appropriate, but it is becoming essential in order to cope with the growing number of very complex intelligence problems that are key to the making of policy decisions. Such an analytical approach will provide the Agency with a finished intelligence product that can best be termed *holistic*.

What are the specific advantages of the polydisciplinary approach to intelligence analysis? First, this approach will make more explicit than is now the case the interrelationships of the various dimensions of complex intelligence problems which are now treated in a fragmentary form or individually as discrete problems. Second, possibly the most important objective of intelligence analysis is to identify the range of possible outcomes of a given situation and to attach some ranking or likelihood to each of them. A polydisciplinary research approach to intelligence problems offers high promise in efforts to achieve this objective. Research to date on polydisciplinary research has shown that:

> The interaction among scientists of different disciplines will result in new combinations of ideas that will not occur in the absence of intense team interaction. This interaction will lead to the asking of questions that would never be asked from a monodisciplinary perspective. And, finally, these new combinations of ideas and the asking of new questions will generate a greater range of proposed solutions to the team problem.[9]

The history of the development of the physical and natural sciences and technology clearly shows that the majority of significant advances were the result of a polydisciplinary research approach. This is no less true in the social sciences where the movement to higher and more sophisticated levels of analysis has been made

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[8]The following terms and definitions will be used throughout the remainder of this paper. They are adopted from the work of Michael Anbar and Bernard Cohen. See Michael Anbar, "The 'Bridge Scientist' and His Role," *Research and Development*, July 1973.

monodisciplinary approach—the analysis approach to a problem from the perspective of one discipline.

polydisciplinary approach—the analytical approach to a problem from the perspective of several disciplines.

The terms multidisciplinary and interdisciplinary refer to two modes of conducting polydisciplinary research.

multidisciplinary mode—a monodisciplinary team leadership formulates the plan of the project and specifies the contribution of each of the participants.

interdisciplinary mode—each of the disciplines represented on the team interacts on an equal footing to formulate the plan of action and to specify the contributions of each of the participants.

[9]Michael Anbar, *Op. cit.*

possible almost exclusively by the integration of concepts and approaches from several disciplines. Such an integrative process has produced the landmark developments in the social sciences such as David Bidney's *Theoretical Anthropology*, the first work providing a general theoretical framework for cultural anthropology, based on work in anthropology, sociology, psychology, philosophy, and history; Morgenstern and von Neuman's work on game theory which drew upon mathematics, philosophy, and economics; Kurt Lewin's *Field Theory in the Social Sciences* which was based upon psychology, mathematics, and sociology; and finally Parsons and Shils' *Toward a General Theory of Action* which was built upon the adaption of concepts and approaches from psychology, sociology, cultural anthropology, and political science. This latter work provided a truly significant theoretical framework for the social sciences in general.

It may be correctly contended that almost all the analytical offices of the Agency are staffed, in varying degrees, with people who have training in various disciplines. The presence, however, of such a staff does not mean that truly polydisciplinary research in under way. Neither does the presence of an inter-office research project task force mean that a polydisciplinary research design for the project has been conceived and is being carried out. Inter-office projects frequently result in the participating offices preparing their contributions for such a study according to their respective missions and special expertise; these submissions are then collated, edited, and organized to make a coherent presentation. Such products, however, do not reflect the influence of a sustained dialogue among a polydisciplinary group of analysts who work together within established conceptual frameworks of analysis which explicitly relate the many aspects of the problems they are addressing. In such an environment, each analyst has an opportunity to acquire a much broader appreciation of a problem as a result of his exposure to the various ways that individuals with different professional backgrounds may approach it.

Such efforts in the Agency have been few and far between but not non-existent. Indeed, a considerable amount of the work performed by the Analysis Division of the Office of Economic Reports (OER—then ORR) in the 1950s and early 1960s on the Soviet and East European economies was based to some extent on a polydisciplinary approach. This Division utilized the narrowly-scoped specialized studies prepared by the engineering, technical, and economic specialists of the other components in the Office in broader analyses considering various facets of the Soviet and East European economies. In the Office of Scientific Intelligence, an attempt was made in the early 1960s to approach the analysis of the Soviet space program on a polydisciplinary basis.[10] The problem was defined and specific pieces of it were assigned to various analysts with the requisite disciplinary backgrounds to deal with them. This analytical program encompassed the research contributions of intelligence officers with backgrounds in the physical, engineering, natural, and social sciences. In this effort, however, there was insufficient interaction among those involved in the project to generate an analytical approach sufficiently sophisticated to encompass the many interactions among the various factors of the problem they addressed. Such attempts as these unfortunately remain exceptions to the overall pattern of intelligence research at the intermediate-analysis level within the Agency.

There is also certainly a need for monodisciplinary intelligence analyses, but such studies need to be conceived within more rigorously developed research designs

[10] *The Soviet Space Research Program Monograph II*, Objectives CIA/SI 32-59 29 August 1959.

which make explicit how the analysis will proceed and on what assumptions it will rest. The spelling out of these "theoretical underpinnings" should include the clear delineation of the conceptual view or model of the system (i.e., the state, the party, the bloc, the weapon system, etc.) being studied, the assumptions that the acceptance of that view imposes, any hypotheses to be investigated, the tests to be employed in establishing their validity, the methods to be used in manipulating the information involved, and a characterization of the data themselves. Unless a study proceeds with some awareness of such considerations, it is unlikely to get beyond the descriptive stage. Because these conceptual foundations are so important, they must be accessible to the reader in explicit form. If a monodisciplinary study is to contribute to polydisciplinary research, its underpinnings must be so well revealed and understood that new and broader concepts for integrating a number of problem factors can be developed. Thus a holistic approach to intelligence analysis will put new demands even upon those studies produced with a single focus.

There are two major reasons for such work. First, there is a need to incorporate into intelligence research additional discipline perspectives, primarily in the behavioral sciences, to deal with the increasing number of questions wherein these disciplines are relevant. As noted above, such disciplines heretofore have not been used to any great extent in CIA analysis. Second, the conduct of monodisciplinary studies from the standpoint of these disciplines would eventually help pave the way for the integration of the contributions that they have to make to the analysis of complex intelligence questions on a polydisciplinary basis.

There has always been, and there remains, a high level of U.S. intelligence interest in various foreign governmental and private institutions and their contributions to the governmental policy-making decision process. Different disciplines provide considerably different analytical perspectives and, therefore, different insights into the roles and internal dynamics of institutions. A sociologist, for example, looks at political parties, political leadership, and bureaucracies in general with significantly different considerations in mind than does a political scientist, historian, or physical scientist. The result is that he conceptually models the problem with which he is working in different ways.[11]

Two recent books, Jean-Claude Thoenig's *L'Ère des Technocrates*, and Jacques-A. Kosciusko-Morizet's *La "Mafia" Polytechnicienne*, are illustrative of the potential utility of a particular type of sociological analysis to intelligence. Thoenig is a sociologist specialized in the sociology of organizations and Kosciusko-Morizet is a scientist-engineer steeped in the literature of the sociology of organizations.

Thoenig's work deals with the role of the corps of engineers for bridges and roads in French public administration and in the broader context of French society. More specifically, he focuses on the evolution of this elite group in French public administration since the 18th century, the recruitment of its members, their educational and social backgrounds and geographical origins; their discipline and cohesiveness; the infrastructure of their own administration; and finally an analysis of the significance of all these variables for the position they occupy in the French government. This position is one that gives them a monopoly of authority over

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[11]For illustrative purposes see: David E. Apter, "A Comparative Method for the Study of Politics," *The American Journal of Sociology*, Vol. LXIV, No. 3, 1958, pp. 221-237; Leonard Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces*, Vol. 27, 1949, pp. 305-310; and Michel Crozier, *The Bureaucratic Phenomenon*, The University of Chicago Press, 1967.

highways, ports, canals, and airports throughout France and, therefore, over the bulk of the French technical civil service at the national, departmental, and local levels. Thoenig assesses the implications of this type of institution and elite for both French public administration and society in general.

Kosciusko-Morizet's book, *La "Mafia" Polytechnicienne,* is a companion volume, in a sense, to Thoenig's work. The author deals with the position of L'École Polytechnique as an institution in France, its history, the role of its graduates in French government administration, its role in the process of elite formation, the place of this elite in the structure of French society, and the implications of their position for the French political system and society in general.

The analytical frameworks of these studies emphasize the systemic and dynamic aspects of institutional behavior; the emphasis is on the *how* rather than the *why* of behavior; and both history and environment are examined to provide insight into the ongoing process of institutional change rather than an explanation of the results of change.[12] This analytical emphasis has especially important implications for intelligence in that it offers much potential for charting and understanding in advance certain processes of change that are likely to produce particular types of institutional behavior.

Despite the problems of data availability that obtain in much intelligence analysis, especially on the closed societies, the approach employed in these two studies suggests an excellent analytical framework for the analysis of the roles of particular elites in various foreign institutions or social sectors. Indeed, though retrospective or historical analysis is something of a luxury in CIA, it might prove useful to undertake a number of such studies of institutions and programs of longstanding intelligence interest. These studies should help improve the analyst's basic understanding of how various foreign institutions function and change.[13] This type of an approach should, over time, move the analysis of foreign organizations and programs away from its predominantly descriptive and *why* orientation to a more analytical and predictive focus that would be valuable for both intelligence analysis and clandestine operations.

It may be argued that the intelligence analyst does not have access to enough data to undertake the types of analyses suggested above. It is a fact, nevertheless, that studies of various institutions and programs are undertaken in CIA; the contention here is simply that better defined research designs going beyond traditional approaches will improve the analysts' capability to make the most of the available data.

Do such approaches or experiments properly belong only in the domain of the academic investigators? The answer must be "no" if the Agency hopes to be prepared to deal effectively with the increasing complexity of national security questions. As

[12] For elaboration on this point see: Michel Crozier, "The Relationships Between Micro and Macrosociology," *Human Relations*, No. 3, Vol. 25, pp. 239-251.

[13] The admonition of the distinguished cultural anthropologist, E. Evans-Pritchard, is apropos on this point. He stated that "the claim that one can understand the functioning of institutions at a certain point in time without knowing how they have come to be what they are . . . seems to me an absurdity." E. Evans-Pritchard, "Social Anthropology: Past and Present," *Man* L, No. 198 (1950). The Marett Lecture, 1950, p. 123.

long ago as 1958, R. A. Random, made the following observation which is most relevant to the argument above:

> To suggest that it is redundant and impractical to erect a science of intelligence is not to reject the application of scientific methodology to intelligence, and specifically the acknowledgement and use of the principles of the social sciences applicable to the phenomena of intelligence. Such a rejection would reject rationality and scientific principle as a basis for practice, and substitute intuitive guesses and unanalyzed conjectures. While irrational conduct of intelligence practice, like non-principled behavior generally, may become skillful and may be successful to the extent of attaining particular ends desired, as a rule it can be recommended only as a kind of short cut in simple situations. When the situation is complicated and the actor is confronted with multiple choices of action, reliance on non-principled behavior introduces an unacceptably high level of probable error.
>
> The propositions advanced above—that it is not profitable to develop intelligence as a separate science because the phenomena with which it deals are covered by the social sciences, and that the only sound practice of intelligence is that based on the scientific method as specifically applied in the social sciences—have important practical implications. The main one of these is that we must build up within the intelligence community a knowledge of scientific method and the techniques and principles of the policy sciences and must study their application to intelligence problems. We must do this because it is the only way to effect any fundamental improvement in professional intelligence practice.[14]

It may be contended that, in general, the level of theory and method in the various social sciences is so primitive that they offer little aid to the intelligence analyst. The rejoinder to this argument must be at least twofold. First, the accuracy of this argument remains largely to be verified empirically in the Agency's intelligence analysis process through experimentation with various theories and methods. Second, granted that social science theory and method are primitive relative to those of the physical and natural sciences, significant progress has been made in developing new approaches to identifying and understanding the immensely complex interrelations that occur among the actors within a given social system.

Although our ability to define mathematically how the effects of a perturbing event will be passed from one element of the system to another is grossly limited, these approaches at least better enable us to understand what is happening. Since particular disciplines (e.g., economics, political science, etc.) tend to limit their attention to only selected types of events and actors in a social system, it is important that we include a number of different disciplinary perspectives and that they be as rigorously defined as the state of the art will allow. Thus, the use of theory and highly structured designs derived from the perspective of different disciplines should expand the spectrum of hypotheses about a given intelligence problem.

There is no intention here to suggest that more attention to theory and research designs in either polydisciplinary or monodisciplinary approaches to intelligence will lead to the methodological rigor that obtains in the physical and natural sciences. On the contrary, it is imperative that those engaged in both the management as well as the conduct of intelligence analysis be alert to the pitfalls of slavish attempts to impose

[14] R.A. Random, "Intelligence as a Science," *Studies in Intelligence, II/2.*

upon the analysis of social science phenomena the methodological rigor that is productive in the physical sciences. Even to entertain such an expectation is to fall victim to scientism.[15]

The Implications of Adopting the Holistic Approach

It is not enough to advocate a major change in a function as important as intelligence analysis without at least identifying some of its salient implications. It must suffice here to outline only those that would seem to be most important if a *holistic* approach were adopted.

Formulation of Intelligence Questions—A decision to take such a step would impact significantly on the types of questions the intelligence community regularly addresses at the three levels of analysis discussed earlier. For example, many of the discrete questions now treated at the intermediate and, to some extent, at the synthesis levels of analysis about such matters as particular aspects of foreign institutions, manpower levels and costs of various economic and scientific research programs, and the performance characteristics of weapon systems would be shifted backward to the "building block" phase of analysis. Clearly, these types of questions are basic and indispensable. With a *holistic* approach, however, such questions would become the underpinnings for the subsequent investigation of broader questions. The effect of this development would be a redefinition of building block studies as a result of the polydisciplinary consideration of more broadly posed intelligence questions.

Requirements and Collection—The adoption of a holistic approach to intelligence analysis would have a significant impact upon that extremely important but frequently neglected relationship between the analysts and the collectors of information. First, the broader focus would result in the examination of problems from different points of view which would in turn generate a different type of intelligence requirement from that which generally has been asked by analysts working predominantly within the framework of a single discipline. Increased emphasis, for example, would be placed upon the interaction and relationships among the variables of a given problem. In essence, the questions would deal more with the way in which various systems operate internally than with the discrete external features. Second, the use of more explicit and theoretically based research designs should result in the better structuring and definition of data requirements to meet the specific needs of the project by highlighting the key categories of data required. Third, the requirements to support a broader analytical approach would require a substantial understanding of the research designs for particular intelligence projects by the collectors of information. All three of these factors would undoubtedly affect the nature of intelligence collection operations and place new demands upon those involved in them.

For example, polydisciplinary intelligence analysis would probably require, over time, innovation in approaches to clandestine intelligence collection. Thus, efforts to collect information about a particular foreign elite's perceptions on important political, economic, or strategic issues might necessitate the use of indirect or clandestine opinion survey research in the target country.

Staffing of Analysis Components—Clearly, the polydisciplinary approach requires an examination of past and present personnel requirements and recruitment policies of

[15]For an excellent discussion of this problem see F. A. von Hayek, "Scientism and the Study of Society," *Economica*, New Series 9 (1942), pp. 267-91; 10 (1943), pp. 34-63; and 11 (1944), pp. 27-39.

the Agency's analysis offices. To approach intelligence analysis on such a basis requires disciplines that are not present in the Agency's analytical staff. The narrow professionalism that has permeated staffing philosophy within the Agency's analysis components must give way to the acceptance of the fact that the ever-increasing interplay among scientific, economic, political, cultural, and strategic variables and the relationship between domestic and external affairs must be viewed at every level of the intelligence analysis process. This view must prevail if the final intelligence product is to be the most useful and relevant that can be provided the policy makers.

Management of analysis—The implementation of a *holistic* approach to intelligence analysis would be a difficult undertaking. It would present major challenges to both the managerial and working levels of both the analytical and collection components. Not the least of these challenges would be the immensely difficult task of reorienting several sectors of a large bureaucracy away from well-established practices to significantly new ways of doing business. For example, it would be necessary for each analyst involved in a polydisciplinary-based project to become very familiar with facets of a given problem other than those in which he or she is a specialist.

A number of significant alterations in the present structure and management of analysis would be required over time as a result of the adoption of a polydisciplinary approach. It would be necessary to develop an organizational approach that would allow the assignment of analysts now working in separate organizational elements to a single analytical task. While organizational changes may contribute to the creation of an environment conducive to polydisciplinary intelligence research, they alone are not adequate for its successful realization. Perhaps more important than organizational change is the philosophical outlook held by the managers and analysts and their commitment to its implementation.

An additional important consideration in any effort to implement polydisciplinary research is that experience elsewhere has revealed that different managerial problems obtain in the multidisciplinary and interdisciplinary approaches to research and that different managerial qualities are needed. There is, for example, a "bridging" role to be carried out by research managers. The need for fulfilling this function helps to identify certain characteristics that managers of polydisciplinary research should possess.

In the preceding issue of **Studies,** *Jack H. Taylor matched the results of NIEs on Soviet weaponry against the thesis of Professor Albert Wohlstetter that the Pentagon track record—public opinion notwithstanding—has been to* **under***estimate rather than* **over***estimate Soviet strategic forces. Taylor found for Wohlstetter's thesis. In the following article Ross Cowey demonstrates that the estimates fared much better in their non-quantitative judgments.*

The Editor

MORE ON THE MILITARY ESTIMATES

Ross Cowey

Having worked closely with Jack Taylor in drafting some of the National Intelligence Estimates which he surveyed in his article in the February *Review of National Intelligence,** I read his piece on Soviet military estimates with great interest. I found myself in agreement with most of his findings, but disappointed by his failure to go farther with some of his analysis.

Taylor's summary of the Estimates reminded me of an observation which Abbot Smith made in an article in the Fall, 1969 issue of *Studies in Intelligence (XIII/4):* "One could easily make up a list of projections (from the military estimates) which were too low, another of those which were too high, another of those which were substantially correct, and a final one—very short—of those which, thanks more to luck than wisdom, were precisely correct." Taylor certainly proves the point.

Admittedly, Taylor focused on the quantitative *under*estimates referred to by Professor Albert Wohlstetter.** What his article does not show, therefore, is that the estimates were right with respect to a number of important, non-quantitative judgments made over the years about Soviet forces. Perhaps the most significant of these was the repeated judgment through the Sixties that the Soviets could not expect to achieve strategic capabilities which would make rational the deliberate initiation of general war.

Any review of the Estimates written since about 1962—i.e., since the advent of improved collection systems—would also show that the intelligence community has been able to provide warning of the introduction of every major Soviet strategic weapon system well before its initial operational capability. We have *not* always been able to agree among ourselves on the specific mission of each new system (e.g., the SA-5 missile and the Backfire bomber) or—as Taylor shows—about the pace or extent of its deployment. But we have been able to provide the planner with knowledge sufficient for general guidance, if not for detailed planning.

I would agree with Taylor that part of the reason for our repeated underestimates in the mid-Sixties of the impending growth in Soviet ICBM forces was an over-reaction by the community to the gross *over*estimates on this subject in the late Fifties. Over-reaction to past mistakes—at least to past overestimates—seems to be a recurring pattern in the estimative process. Our overestimates in the mid-to-late

*See also *Studies in Intelligence, XIX/1.*

**"Is There a Strategic Arms Race," *Foreign Policy,* Summer 1974.

Fifties of Soviet missile, bomber, and fighter strengths were followed by underestimates for each of those forces during the Sixties.

Another analytical syndrome working against us—one which may be even more controlling—is our tendency to overestimate future force levels in the absence of firm evidence, and to underestimate with the advantage of such evidence. During the late Fifties, we were groping in the dark for information on what the Soviets were doing as they translated the new technology of the space age into new military hardware. With the introduction of more sophisticated intelligence collection methods in the Sixties, we gained a much better appreciation of Soviet capabilities to make use of the new technology. But this more complete base of information led to more conservative analysis, and to consistent underestimating. The *less* information we had, the more we *over*estimated; the *more* information we had, the more we *under*estimated. We hedged against uncertainty, but felt constrained by evidence. This is not to say we would have been better off with less information. What it does say is that we should guard against this tendency to overestimate in the absence of hard evidence, and to underestimate in its presence. Another phenomenon to which we seem to fall prey is the one to which Wohlstetter refers in his article, and which Taylor cites: the intrinsic uncertainty of predicting the size and mixture of a deployment program, because decisions on size or mix can be reversed between the time of our prediction and the time of actual deployment. In 1958, for example, we overestimated the strength of Soviet fighter forces for the early Sixties, but this resulted mainly from Khrushchev's unanticipated decision in the interim to cut back Soviet general purpose forces in favor of missiles.

Indeed, our estimates—right or wrong—can in themselves have an impact on force-level decisions, in both the U.S. and the USSR. The infamous "missile gap" gave strong impetus to U.S. strategic weapon programs, which contributed at least indirectly to Khrushchev's decision to put strategic missiles into Cuba. Soviet embarrassment in Cuba in turn gave impetus to the USSR's strategic weapon programs. The ensuing underestimates of the growth in Soviet ICBM forces resulted at least partly from our failure to take full account of this action-reaction phenomenon. The full effect of such interactions is so unpredictable, however, as to make complete accounting difficult if not impossible—even in retrospect.

In the submarine force estimates, we ran into a different problem: mirror-imaging—the tendency to use American experience as the means to measure likely Soviet goals. In the early Sixties, we estimated (without any direct evidence) that the USSR would follow the U.S. lead and build a sizable force of ballistic missile submarines. What we did not recognize at the time was that the Soviets saw a need for more cruise missile submarines—to defend themselves against U.S. aircraft carriers—and that the Russians were having difficulty developing an acceptable ballistic missile submarine system. The result was that full-scale production of modern ballistic missile submarines did not start in the USSR until the mid-Sixties, which put our estimates of the early Sixties way over the mark. I would like to think that the later and correct five-year estimates of 1968 and 1969 resulted from my having written them, but in reality they resulted merely from straight line projections of identifiable production rates—on up to the "mirror-imaged" and now demonstrably low estimate of as many as 50 modern ballistic missile submarines, a figure we then believed to be the ultimate Soviet goal.

The list of quantitative errors, then, is a long one. But this, perhaps, is not so surprising, considering the number of specific estimates made and the limited amount of information available to us at the time they were made—at least in the early part of

the period. The future is not likely to be any brighter. We will not be wanting for sophisticated intelligence collection systems, but difficult-to-observe qualitative improvements in the weapons already deployed will be as important to us in the future as changes in the observable number of delivery vehicles have been in the past.

In an arms control environment, where most of the developments prohibited by a treaty will be those which are relatively easy to monitor, we will not only have to watch for violations of the agreements themselves but will have to try to follow the variety of more difficult-to-observe improvements in Soviet weapon systems which will be permitted and which are likely to proliferate under such agreements. More than ever, the task for intelligence will be to observe the unobservable—and as the Soviets become more cognizant of our intelligence sources and methods, more things are likely to become less observable.

For the preceding issue, Robert M. Clark provided a layman's guide to the birth, developments, and some of the basic tenets of Scientific and Technical Intelligence, possibly with a somewhat jaundiced eye and with tongue rather firmly in cheek. In the interest of balance, we provide the answer he provoked from fellow S&Tman Donald C. Brown.

The Editor

ANOTHER VIEW OF S&T ANALYSIS

Donald C. Brown

It was with a feeling akin to *déja vu* that I read Dr. Clark's article. What he describes is very familiar, but it just isn't the S&T intelligence that I know.

With the large variety of individuals, government agencies, quasi-official bodies, segments of private industry, and an assortment of fans involved in some way with scientific and technical intelligence, it is not surprising that there are so many views on what this area of the intelligence art *is*. Perhaps Dr. Clark's article can serve as a starting point for a more rigorous development of the philosophy of what technical intelligence *ought to be*. *Studies*, as the professional journal of Intelligence, would fill a real need by opening its pages to such a continuing dialogue. I shall return to this point later with some suggestions.

Although I think I agree with many of the things Bob Clark says, I profoundly disagree with others, and all in all I must conclude that he and I are viewing S&T Intelligence darkly through different glasses. His view appears to me in part to be too simplistic, in part to dwell on peripheral issues at the expense of the central point, and in part to be just plain wrong. A cynicism which I believe to be unwarranted crops up here and there against the intelligence "outsider," also.

A few of Clark's specific points bear discussion.

To Describe a Weapon is to Know All

Early on in the article, we come upon the surprising statement, "Once you know the characteristics of an enemy weapon system then his tactics and strategy for using the weapon system follow naturally" (!). It would be useful to know more clearly what the author means by tactics and strategy, but this statement does little justice to the complex distinctions between capabilities and intentions. The example the author uses, of the ICBM accuracy needed to disable Minuteman missile silos, appears plausible at first glance, but one need only follow the debate generated during the past year by Dr. Schlesinger's public musings on the proper use of this country's very accurate missiles to recognize that strategy and tactics are not wholly determined by a weapon's technical characteristics.

I would agree that it should be a goal of S&T Intelligence to describe weapon system characteristics, but there is much more to assessing the meaning of the system than sheer mechanical description.

Proving A Negative is a Fool's Errand

Under "Case #2" ("we develop weapons—they don't develop weapons"), the author cites several examples of estimates made in the absence of real evidence, then

complains about the difficulties of refuting such assertions. I suppose all of us analysts have complained at some point in our careers about this problem. The issue in my view, however, is not, how do we suppress the asserters?, but, how can S&T intelligence improve its capability to deal with important questions? Certainly, if we concern ourselves only with questions on which we have "adequate" information we will be in danger of propagating a distorted picture. There is much that we can do in the way of identifying the information needed to answer important questions, and much that we can do to collect it, but there will always be inadequate information. Someone needs to give some deep thought to a better way of usefully illuminating the issues for which hard intelligence information does not exist.

We Tend to Ignore Developments That Don't Mirror Our Own

In "Case #3" ("we don't develop weapons—they develop weapons"), Clark oversimplifies the history of U.S. intelligence interest in anti-ship cruise missiles by confusing the missile with the system. It is true that the threat posed to U.S. naval forces is better appreciated today than it was in the early 1960s, but that is because the threat is greater, not necessarily because the intelligence community was slow on the uptake. It takes more than a missile to threaten a ship: the missile must first be placed within firing range of the ship and provided with a knowledge of where it should go to hit the ship. In truth, the Soviet anti-ship missile forces did not pose a serious threat to the U.S. surface fleet 15 years ago. The fact that they do today is attributable to the tremendous growth and expansion of deployment of the Soviet navy and the consequent growth of total cruise missile *system* capabilities—launch platforms, targeting systems, and communications, as well as missiles.

There is nevertheless a lesson in this example. S&T Intelligence can and must do a better job of anticipating problems and putting itself in a position to cope with them when they become real.

Phantoms in the Night

I'm not sure what point Clark is trying to make in his discussion of "Case #4" ("we don't develop weapons—they don't develop weapons"), but I gather that he feels frustrated answering the "what if . . . ?" questions such as the one on SAM upgrade.

On the contrary, I feel that CIA's S&T Intelligence took a large step toward maturity as an analytic discipline during the SAM Upgrade Era of the late 1960s. It was certainly an unconventional issue in its time and one not without its frustrations. (One of my favorite memories is of a non-senior CIA official summing up his exasperation before an august review panel with the not-quite-technical argument, "What I don't understand is how the U.S. can't build an effective ABM system after spending ten years and billions of dollars, and you think the Soviets can do it with a bunch of tin cans.") But those of us who were intimately involved in the issue are a little proud that we were pushed by our management into wading into an argument that went against our intuition (a "stupid" hypothesis), and treating it from a strictly analytical viewpoint. It was a tough problem, and the stakes were potentially high, but isn't that exactly the sort of thing S&T Intelligence should be doing—reducing the uncertainties in the information used by policy makers.

The lesson I carried away from the SAM Upgrade Blues is that S&T Intelligence must be more open to unconventional approaches, and we must always be prepared to make our case on hard-headed technical analysis and not on emotion. Steps have

already been taken within the DDS&T to examine such "wild" schemes ourselves rather than waiting to be surprised by outsiders.

The Truth Shall Make You Free

Clark says, "The objective of any intelligence analysis is the *truth*. . . ." That's a pretty highfalutin notion of our calling, especially since I think most people would be hard pressed to say what the truth might be in most of the issues on which we work. I certainly don't want to see the experiments which demonstrate the truth about the accuracy of Soviet ICBMs against the U.S. or the number of Minuteman reentry vehicles the Moscow ABM system can intercept.

I would prefer to see S&T Intelligence aim for something a little more attainable—and understandable. Part of that objective might be to describe and analyze foreign weapon systems and technologies in a way that is of most use to U.S. policy makers. How to meet this goal should be the subject of prolonged discussion, but I think a central feature of any attempt should be to erect a rigorous logical framework for each analysis so that the user can clearly understand its underlying assumptions and the limits to its utility. I am reasonably sure of one thing: We can only reduce the value of our efforts by any pretense that we are revealing abstract truth.

Inexpert Experts

On the subject of experts, Clark seems to miss the forest for the trees. By his definition, I gather, the expert is the tradition of someone from out of town who carries a briefcase. He should be more selective. The test of expertise is not hard to apply, and experts in and out of the official intelligence community have contributed in every facet of our work.

The tale of the ABM radar used to illustrate the caution about experts seems to me to have most of the characteristics of a shaggy dog story. All intelligence problems include different and often contradictory hypotheses at first (else, would they be problems)? As evidence accumulates (in the case of the ABM radar, as construction progressed), we hope we can narrow the bounds on the uncertainty. But what is the point of the tale? I contend that in the intelligence trade it is of little value to be right if your reasoning is not persuasive, and apparently, in Clark's example, those who eventually proved correct about the radar were unable to establish their case persuasively. Even guessers can be correct, but we are not in the business of guessing.

Those Perfidious Contractors

The section on contractors illustrates the ancient maxim: All generalizations are wrong, including this one. It is not hard to understand, when one is exposed to a philosophy as cynical as this, why Clark has had little success with contractors.

Of course a contractor is in the business for the money, much as Clark and I always cash our pay checks. If you can clearly define a problem which you can't solve yourself, if you can make a contractor understand your problem, if you have investigated the contractor's capabilities thoroughly enough to convince yourself that he can solve the problem, and if you supply him with the needed information, then you will probably be satisfied with the results. If you can't do those things, then you have no right to expect good results.

A Proposal

Scientific and Technical Intelligence has come a long way since the beginning days described in the opening of Dr. Clark's article. In the scope and importance of problems addressed and in the number of people engaged in it, it is a major influence in governmental decision making. In the quality and sophistication of some of its work, S&T Intelligence has approached the status of a major scientific discipline.

What is sorely needed to insure continued advances in S&T Intelligence, it has long seemed to me, is a critical mechanism—some means of defining standards and systematically judging our work against those standards.

Formal criticism exists in most other fields of human endeavor. The arts and literature, journalism, science, foreign policy, architecture, you name it—all have an established procedure, be it internal or external, for criticism. S&T Intelligence, and indeed intelligence as a whole, is mature enough—and Lord knows, influential enough in this country—to benefit as much from formal criticism as other disciplines.

Dr. Clark, whether intentionally or not, touches at many points in his article, on the need for criticism in intelligence. How are we (or more importantly our consumers) to tell the good from the bad, the assertive from the analytical, the casual from the rigorous, the valuable from the misdirected?

I hope the editors of *Studies* will join me in calling for a discussion in these pages of the need for criticism in intelligence and the form it should take.

THE LAMS STORY

Martin C. Elkes

The introduction of a new technical capability into an operating component is a long and difficult process; whether it be the introduction of a new rifle (Armalite Model 15 or M-16) into Vietnam, an F-111 aircraft into the Air Force, or an improved navigation system for Agency field use. For various reasons, only a small percentage of the new so-called "good ideas" identified in research or operations actually get into the field as operational gear. It is only natural that many people and organizations have a great skepticism and show considerable caution in supporting new ideas prior to their successful demonstration.

Among the factors which can have an important bearing on whether or not a risky research and development program reaches fruition and is deployed in the field are the people associated with the program. Sometimes a good program survives this skepticism and caution mainly because of the drive, persistence, skill, conviction, and understanding of the people involved. The LAMS –LORAN Airborne Modular System –story is a case in point.

The Requirements

In Southeast Asia in the mid-sixties, the Agency needed a rapid means for determining where and when our aircraft went down, so that the crews could be rescued more quickly. Agency personnel needed to know the exact positions of small indigenous teams reporting the movement of enemy troops and supplies. Agency aircraft needed a more precise position location and navigation system for infiltration and exfiltration of personnel, for more efficient aerial photographic mapping, and for dropping supplies under nighttime or IFR (Instrument Flight Rules) conditions. The Agency's Marine Branch needed a better navigation system for small craft and a better way of coordinating the air and marine operation at night. These situations required smaller, lighter, less complicated equipment, preferably at lower cost. Ground support for the equipment had to be minimal, and the equipment had to be accurate at long ranges and low altitudes. These needs were perceived by Agency personnel in the field and at Headquarters, but they were not formally documented and published requirements at the time. In addition, the Office of Research and Development, DD/S&T, needed even smaller navigation systems for the emplacement vehicles (small, unmanned airplanes, blimps, helicopters, ground-roving vehicles, boats, and submersibles) then being developed.

State of the Art of Navigation in the Mid-1960s

A number of different navigation systems were in use or under development in the mid-1960s. Some of these had features which would meet some of the Agency operational requirements but none of them had enough of the desired characteristics. TRANSIT navigation satellites were being launched and navigation sets were being

built, but they were too large, too heavy, and required too much human attention to get a position "fix." The OMEGA system was being partially implemented by the Navy; that system was planned eventually for world-wide coverage, but it would only provide position accuracies to thousands of meters, instead of to the hundreds of meters or less needed by the Agency. At that time the receiving equipment was too large and too heavy. The DECCA navigation system had a number of chains or ground stations installed, primarily in Europe, but the range over which it was useful was much too small for Agency use. The Air Force's TACAN/VORTAC navigation systems were not useful at the low altitude at long ranges required by Agency operations. The inertial navigation systems were too large and drifted too much. The LORAN equipment in the Air Force planes was too large and unsuitable, although its accuracy was acceptable. The LORAN ground stations installed and operated in Southeast Asia by the Coast Guard and the USAF provided coverage for the entire area.

Since navigation was such an important problem for DoD, there was some improvement work going on in each of the systems enumerated above but none of the systems was being developed quickly enough or in a configuration to meet Agency needs.

The Approaches

Because equipment to meet Agency needs was beyond the state of the art and the technical risks were significant, several parallel approaches were undertaken. Different groups within ORD funded LORAN, TRANSIT, and OMEGA research after a large number of other systems had been considered. Of these three systems, navigation sets using the LORAN system were eventually developed and employed by the Agency in Southeast Asia. The remainder of this article will be about that program.

During ORD's technological survey in 1967, it was found that Teledyne Systems Corporation (TSC) was building a breadboard model to test a promising new concept for LORAN receivers. Their approach would lead to a navigation system design considerably smaller and lighter than the more conventional approaches, if they could make it work adequately. The proposed equipment appeared to meet some of the requirements for team location and downed aircraft location which had been discussed in October and November 1967. Attending were representatives from the Office of the Special Assistant for Vietnamese Affairs (SAVA), Special Operations Division (SOD/DDP, now SOD/DDO), Technical Services Division (TSD/DDP, now OTS/DDS&T), Far East Division (FE/DDP), and ORD/DDS&T. Further refinements of the requirements were made, and possible solutions were discussed from November 1967 through February 1968, inside and outside of the Agency.

To firm up the requirements and to initiate tests, ORD funded a small measurement program to assess the capabilities of the Teledyne breadboard equipment. By April 1968 the results of the first field test were available and were presented to Agency as well as Army, Air Force, and the Naval Applied Science Laboratory personnel. Based on the results of the test, ORD recommended the research and development of a prototype manpack LORAN receiver. In May 1968 the Special Assistant for Vietnamese Affairs formally requested the development of the receiver, and the contract was started that month. The equipment was to be 60 cubic inches in size and weigh approximately three pounds. Because of technical difficulties, the size grew to 132 cubic inches, the weight to five and a half pounds, and

the delivery was seven months later than originally scheduled; but even with that size and weight growth, the receiver was only one-tenth of the size and weight of the Air Force's LORAN sets, the ARN-92 and ARN-85.

Immediately following delivery of the models, operational tests were conducted in the United States and abroad. During the period December 1969 through March 1970, the equipment was tested in the following LORAN chains: U.S. East Coast, North Atlantic, Norwegian Sea, Mediterranean Sea, Northwest Pacific, and Southeast Asia. In Southeast Asia tests were made at Ubon and Nakhon Phanom, Thailand; Pleiku, Cam Ranh, Chu Lai, and Ton Son Nhut in South Vietnam; and in Laos. In addition to testing the manpack LORAN system on the ground in Southeast Asia, it was tested aboard an H-34 helicopter, using a short-whip antenna extended out the door. The helicopter tests were made at Udorn, Thailand, at Saravane, Laos, and in a deep valley about 15 miles from Saravane. To further check the performance in realistic operational situations, the equipment was tested on a rubber tree plantation near Saigon and in rugged mountain terrain near Long Tieng in Laos.

25X1 The equipment was described and shown in operation [redacted] Defense Department headquarters.

The complete ARN-92 LORAN receivers were costing the Air Force approximately $90,000, plus $70,000 to $100,000 each for installation. ORD felt that it was essential that the unit cost for Agency LORAN sets be significantly less than that (in the range of $1,000 to $15,000) to have much chance of being widely used for such applications as indigenous observation teams, aircraft tracking, and marine operations. The most important sub-systems could be common to Agency, Defense, and civilian systems. One way to get the price down was to get the production up on those sub-systems for mass production economies. The Agency production quantities would not be large enough by themselves; therefore, it was considered essential to get other users involved in the production and development of LORAN subsystems the Agency could use. To this end, ORD, the Agency, and Teledyne described the equipment and the tests to as many potential users as they could.

Shortly after the field tests, a cable was received from [redacted] 25X1 requesting assistance in improving the night-drop mission. That mission was to fly over a specified point on the ground at night, in or near hostile territory, drop supplies, precisely, and return to base safely. SOD, TSD, SAVA, and ORD representatives met to discuss the request and recommend a course of action. A contract incorporating these requirements was let in May 1970 with Teledyne for the development of two airborne LORAN line-of-position (LOP) systems to provide Agency aircraft with a precision drop capability. The LORAN receivers developed under the previous contract were incorporated into these LOP's. These equipments were delivered in the fall of 1970 and tested in the Virginia area, in Southeast Asia, and at Eglin Air Force Base. Again, a joint ORD-SOD-TSC team tested the equipment. The equipment was tested in an H-34 helicopter, a C-123, a Beech Baron, a Twin Otter, and a Porter aircraft for a total flight operational time of nine hours in Thailand and Laos. The accuracy attained with these aircraft with the LOP equipment was consistently better than 70 meters. Perhaps the most impressive demonstration of the instrument's accuracy was the blind operational air drop made at a mountain top site in Laos near Long Tieng. Four packages were parachuted into a drop zone approximately 30 meters by 40 meters—without seeing the drop

zone—from a Twin Otter aircraft from an altitude of about 500 feet. At Eglin Air Force Base additional accuracy and drop tests were made, including drops from Agency aircraft to Agency marine craft operated by SOD.

Following these successful tests, a contract was let in February 1971 for the development of 10 LORAN Airborne Modular Systems (LAMS) and auxiliary equipment for use in Southeast Asia. The requirements were backed by SAVA and DDP, the funding was provided by DDP, and the technical supervision was by ORD/DDS&T. This improved LAMS system provided an accurate position location system, a preprogrammed flight plan capability, a flight-following capability, and "time-to-go" indicators. The flight-following facility permitted a ground station to know accurately at all times the position of the aircraft. The "time-to-go" indicators provided the pilot with an accurate indication of the time remaining before the aircraft passed over a preprogrammed check point or the target. The equipment features needed by the Agency were provided in a small and light enough form so that the equipment could be used by Agency aircraft on their unique missions.

After seemingly insurmountable technical, financial, and contractual problems too complex to enumerate, the dedicated DD/S&T-DDP-SAVA team finally deployed units in Southeast Asia in 1972. SOD/Air Branch took the first equipment [redacted] in the spring of 1972 for tests and operational use. 25X1

Operations

Since its deployment in SEA, the LAMS aircraft equipment has been used extensively in the F-58T, the CH-47, the H-34, and the H-500 for helicopter infiltration and extraction; in the Twin Otter, Caribou, C-47, C-123, Porter, and C-130 for resupply; and the VOLPAR for photo-reconnaissance. The navigation equipment was moved from one aircraft to another as the need arose, because there were not enough navigation systems to equip all aircraft. The LAMS sets were averaging 64 hours flight time per month per set.

Some examples of typical operational applications of the LAMS equipment were as follows:

a) *A Specific C-130 Aerial Delivery Resupply Mission.* A mission was ordered to resupply an operational site in Laos which was under attack by hostile forces at the time. The LAMS equipment was installed in C-130 aircraft in 28 minutes. The crew, which had never used the LAMS equipment prior to this mission, was given a one-hour briefing using a video tape and TV monitor to show them how the equipment functioned and was to be employed. The mission was flown during daylight hours under IFR conditions; a rain storm had moved into the drop zone area and the crew had no visual contact with the ground at any time during the drop. Four check points and the drop point were programmed into the LAMS computer. The initial drop was made using only the LAMS. Radio contact with the ground confirmed that the bundle landed 60 meters north of the drop zone. Corrections were made in the LORAN coordinates and a new drop point was programmed. Five more drops were made and all five landed on the drop zone. The LAMS was then programmed for the return flight to home base and the aircraft returned safely.

b) *Resupply of Skyline Ridge Outposts.* The defense perimeter outposts on Skyline Ridge, overlooking [redacted] Long Tieng, were resupplied 25X1

regularly by LAMS-equipped Twin Otters during battles for the base in 1972. The aircraft flew low, between mountains, from a more southern base to a point near Long Tieng. The aircraft then flew a direct line, preprogrammed into the LAMS, and literally climbed the mountain. As the aircraft crested the Skyline Ridge, it made a steep climb, a "U" turn, dropped the supplies to an outpost, and then ducked below the crest of the mountain. Without such a precision navigation system, air resupply of Long Tieng for a three-month period of fighting in mid-1972 would have been far more difficult.

c) *Caribou Night Resupply Missions.* The LAMS equipment was regularly utilized in Caribou aircraft for resupply missions. Usually the Caribou would resupply several teams on one night mission by programming into the equipment the LORAN coordinates for each drop zone. The aircraft navigated to each drop zone, made one pass to drop the supplies, and then proceeded to the next programmed drop zone. This proved to be a secure and relatively safe method of resupply in hostile areas at night.

d) *Twin Otter Resupply Missions.* The LAMS equipment was installed in Twin Otter aircraft for the purpose of resupplying operational teams in remote and mountainous areas lacking easily identifiable ground features. The map coordinates of the ground team were converted to the LORAN coordinates and programmed into the LAMS computer. This system guided the pilot to the team's general location. When the aircraft arrived in the area, air-ground radio contact was established with the team to confirm the exact location. Ground signals, such as smoke or panels, were then placed in the drop zone for the aircraft to make the drop. This close positioning of the aircraft, usually within a quarter of a mile, eliminated the necessity of flying search patterns to find the teams and thus reduced the exposure of the aircraft and the ground team to hostile action.

e) *Helicopter Missions.* The LAMS was the primary navigational aid on helicopters flown in weather and visibility conditions which otherwise would have caused cancellation of the mission. The LAMS equipment was used to locate the helicopter landing zone on infiltrations/extractions of both personnel and supplies.

f) *Photo Missions.* The LAMS equipment was installed in VOLPAR aircraft utilized for photographic missions. During much of the year, climatic conditions prevented the air crews from effectively using visual references to navigate to the mission area, so the LAMS was used. Once the photo mission area was reached, the LAMS equipment was programmed to navigate each photographic leg and to print the LORAN coordinates on the film. The improved precision in flying the legs saved much film and time and reduced exposure of the aircraft. When the mission was completed, the LAMS equipment was programmed for the return flight to home base.

The LAMS story is a good illustration of what can be done by a small group of individuals banded together voluntarily in an interdirectorate team to increase the Agency's capability in a field operational situation. In particular, the coordinated and cooperative approach taken by a group of key players from SAVA, the Special Operations Division of the DDP and ORD was a key to the success of this project. They worked together in the face of many difficulties with an outstanding *esprit de corps,* inspired by dedication to their common goal.

Epilogue:

The LAMS equipment is still in use; it will be refurbished and continued in service. If the Agency had waited for others to produce the LAMS or its equivalent, it would still be waiting.

A prototype of a subminiature LORAN receiver (the size of a package of cigarettes) has been developed by the Agency as a follow-on; it is expected to be a model widely used for Agency, Defense and civilian applications. The Coast Guard and the Agency jointly now are funding the development of a subminiature, improved, LAMS-like equipment incorporating the subminiature receiver.

INTELLIGENCE IN RECENT PUBLIC LITERATURE

(Editor's Note: The following is primarily a review of the book, and does not purport to examine the possibility or extent of Soviet involvement in Agee's actions, from the start or at an early stage.)

INSIDE THE COMPANY: CIA DIARY. By *Philip Agee*. (Penguin Books, 1975.)

Philip Agee's 600-page story of his career and views as a junior and middle-level case officer in Quito, Montevideo, and Mexico City will anger all those who have worked for the Central Intelligence Agency because he is its first real defector in the classic sense of the word. Though it is unlikely that he could be successfully prosecuted in a cold or at least cooling war, in a hotter context Agee would fall into the area which the Constitution, speaking of enemies in time of war, defines as "giving them aid and comfort." In any case *Inside the Company: CIA Diary* will certainly give aid and comfort to any one looking for concrete and heretofore classified information about some aspects of the Clandestine Service. Unlike previous information about CIA operations made available by Victor Marchetti and others who have claimed to have had the best interests of the country at heart, this book aims, Mr. Agee says, to get "useful information on the CIA to revolutionary organizations that could use it."

Agee begins with an account of his recruitment in 1959 and his training in the Career Trainee program. His 50-page recitation of the instruction he received is an accurate description of the intelligence community, the CIA structure, and the doctrines, tradecraft, and terms of the Clandestine Service. He then devotes 216 pages to his tour of duty in the Embassy in Quito, 1960-1963, almost as many pages to Montevideo, 1964-1966, and 64 pages to his final tour in Mexico City, 1966-1968. He introduces each of his three tours with the headquarters appreciation of the local operating climate, a description of the political parties, and, except for Mexico, the CIA's objectives in the area (the Related Mission Directives). His most thorough revelation of sensitive information is given in his accurate descriptions of each station's operations under identifying cryptonym.

After establishing this very complete background, Agee publishes what appear to be chronological diary entries which describe his operations and their progress, other station operations, and new operational initiatives as they developed. However, whatever factual information may have been contained in his actual diary, the entries now have been expanded to include the historical, political and economic contexts of his operations as he now views those contexts since leaving the Agency. Thus, what we have in this book is not a diary of the period, but an account of that period interpreted after four years of subsequent research, and evaluated by very different ideas and attitudes than those he held at the time. Agee makes no attempt to conceal his methods of composition, but what he presents in the form and rhetoric of his restructured recollections is a "diary" that sounds more authoritative, comprehensive, and intelligible than any diary actually kept by a professional in similar circumstances could possibly have been.

Agee's personal story as he now sets it forth is that, upon joining the Agency as a "patriot dedicated to the preservation of my country and our way of life," he readily accepted the policy that some covert extension of the national effort to counter

Communist expansion was desirable in order to allow political forces to evolve a better society. He finished his three and one half years in Quito in tune with the station program. He wanted to resign by the end of his Montevideo assignment, however, because—he says—he arrived slowly at the conclusion that the U.S. role in Latin America, while superficially well intended, perpetuated injustice rather than reducing it. In Mexico City his increasing dissent amounts to defection. After resigning from the Agency in Mexico City, he cast about for other employment. His need to earn a living became acute, and the writing of this book appears to have been a solution to that problem. In the last and briefest part of the book he shares some of his new economic views on Latin America and describes some of his steps and problems between 1970 and 1974 in preparing and publishing his "diary."

The effect of the publication of the classified information in his book is clearest in its damaging impact on CIA's activities and persons with whom it dealt in Ecuador, Uruguay, and Mexico. Those whose interests lie in identifying and neutralizing U.S. covert action will find it useful, especially the alphabetized Appendix of 429 names and descriptions of "CIA Employees, Agents, Collaborators and Organizations," largely in Latin America. He does not discuss specific projects or identify agents in other geographic areas, though some of the text could be used to identify operations outside Latin America, such as CIA's international security cooperation, Labor, Division D, and UN operations. His description of the Clandestine Service's *modus operandi* is valid outside Latin America, and Agee is said to be working on a larger project describing CIA activities all over the world. I would assume that he has prepared a long extension of the Appendix name list, with the new title of "Probable and possible CIA employees, Agents, Collaborators and organizations," and that such a list would be extremely useful to other intelligence organizations. However, I would judge that most Latin readers will perceive his revelations in context with the Soviet and other nations' activities and within the concrete realities of their own continuing struggles, and that they will express no broad new surge of moral revulsion against the U.S. The book's main achievement is to provide the Communists and extreme Left with specific knowledge of CIA's Latin American operations and insight into CIA *modus operandi* in order to permit them to counter U.S. and particular CIA actions. As such, it will doubtless make the required reading list of the KGB mid-career course. The book will, of course, disillusion some U.S. readers and will doubtless be used to support some "causes." It may also, however, educate the broader public beneficially on the subject of secret intelligence.

The book will affect the CIA as a severe body blow does any living organism: some parts obviously will be affected more than others, but the health of the whole is bound to suffer. A considerable number of CIA personnel must be diverted from their normal duties to undertake the meticulous and time-consuming task of repairing the damage done to its Latin American program, and to see what can be done to help those injured by the author's revelations.

Agee's knowledge of local personalities and history is impressive. I have no great quarrel with his reporting and analysis of events, though I remain uneasy as to the extent of the bias introduced into his recollections from 1970-1974 by his research in institutions in Mexico City, Havana, Paris and London, as well as by his strong, but not ultimately clear, attitudes and feelings about his own past or the world about him. Nor do I fault his concern (shared by the U.S. Government) for the unequal distribution of income in Latin America, a point he returns to frequently. I can even stay with him as he claims that U.S. policies do not always deal fully with injustices.

Apart from a subtle pervasive imbalance in judgment stemming from the fact he writes his book long after his conversion, other problems arise because his economic analysis is stereotypically Marxist. He gives undue emphasis to the reporting of violence. He is over-impressed with the possible impact of CIA operations on public affairs. Out of the factual material he provides, someone else might have written a critique of the Alliance for Progress which might have favorable influence on U.S. policy. With only hints as to the depth of his dissatisfaction, however, he sets forth on page 503 a fantasied letter to the Director, in which he says, "Our (the U.S.) only alternatives are to continue to support injustice or withdraw and let the cards fall by themselves . . . it is clear that the only real solutions are those advocated by the Communists and others of the extreme left . . ."

Rather than "withdraw and let the cards fall by themselves," he takes an emotional leap, committing his personal energy to use the knowledge he has come to possess against the value structure he has been part of. However, he is not really interested in telling us much about this. His account in the last part from his defection to the publishing of his book is sanitized and as devoid of the names of those he dealt with as the earlier parts are full of them. That he omits part of his story is patent. One assumes that when he visited Havana he received editorial assistance from the Cubans and Soviets, but how much is, of course, not clear. .

This book will not be of as much interest to Agee's former colleagues in the Agency as he might imagine. Agee gives no professional account of operations, *per se,* judgments in the inception of operations, agent motivations, or the effectiveness of different operations. Nor does he consider or speculate about CIA thinking and judgment at operational echelons higher than his junior level. He provides little insight into his relations with colleagues and agents, nor is he candid enough about his tergiversation to be thought-provoking. Such additions, larded with a little humor, would have made it the best-yet story of life as a case officer. As for the themes which would surely attract serious writers on this topic, he sheds no fresh light on human behavior, international relations or the role of intelligence in a democracy. Nor does he give us any good reading on our paramount interest—why (and how), after becoming disenchanted with his work, does a case officer fully aware of Soviet history and practice, take the course of acknowledged betrayal?

Agee appears personally to have been compatible with his colleagues as a case officer, to have competed well with his peers, and to have held a conventional political outlook. He observed in an alleged diary entry dated 1968 that he feels "unsure about finding satisfactory work inside the same system rejected long ago as a university student." Yet after college he appears to have stayed in the system, joining the CIA for patriotic reasons, involving himself in sophisticated political operations, and as he puts it, becoming one of capitalism's secret police. In Mexico in 1971 he reports a further change in his political views (page 564) saying, "The key to adopting increasingly radical views has been my fuller comprehension of the class divisions of capitalistic society based on property . . . that class identity comes before nationality. . . ." This time he decides to take action ". . . to name all the names and organizations connected with CIA operations . . . to convey them to revolutionary organizations that could use it to defend themselves better." He expresses neither pleasure nor concern that this step carries with it his rejection of his colleagues and country. He certainly emerges as a person with shallow attachments.

In addition to rejecting the political system he had been a part of, Agee earlier had rejected going into his family business, had left law school after three months,

and left his wife and later his girl friend. He says in January 1971 "I begin again after a year of great disappointment and sense of failure. My hopes for a new start and future in Mexico were clouded with the failure of my marriage plans, and I am unsure of my direction. The reasons are a complex series of mistakes, perhaps even unrealistic hopes from the beginning, but with results too damaging to overcome." Whether one's hopes are realistic or unrealistic, the pain and anger caused by recurring disappointments are intense and pervasive, yet he is inarticulate about his deep sense of personal injury. He gropes to express his values associated with defection in these words:

"There is a contradiction in what I am doing but I don't have much choice given the plans we have and our need for income. One has to take the realistic view: in order to fulfill responsibilities you have to compromise with the system knowing full well that the system doesn't work for everybody. This means everybody has to get what he can within decency's limits—which can be stretched when needed to assure a little more security. What I have to do now is get mine, inside the system and forget I ever worked for the CIA. No, there's no use trying to change the system. What happened at the Plaza of the Three Cultures is happening all over the world to people trying to change the system. Life is too short and has too many delights that might be missed. At thirty-three I've got half a life time to enjoy them."

He gives up any pretension to idealism. Is this not the mercenary saying "Because I find there is no use in trying to change an iniquitous system, I shall become iniquitous myself in order to obtain the satisfactions I desire"? Like other adolescents of the 1960s who have vented their impatient and frustrated idealism in destructive acts, Agee, out of touch with his deeper feelings, vents his rage and displays a towering arrogance. By virtue of the trust placed in him, he damages more than himself.

THE SHADOW WAR: EUROPEAN RESISTANCE 1939-1945. By *Henri Michel.* (Harper and Row, New York, 416 pp.)

This is the first publication in English of any major work by Professor Henri Michel.* He is an indefatigable worker, with a lengthy list of titles to his credit, by himself or as editor or co-author, to say nothing of numerous periodical articles. Although he was not a member of the French Resistance himself, the bulk of his writing has been in the field of the World War II French Resistance with occasional forays into resistance movements in other European countries. This reviewer has known Michel more than 15 years, a good portion of the more than 20 years that Michel has served as the Secretary General of the official French Comité d'Histoire de la Deuxiéme Guerre Mondiale. In this capacity Michel has supervised a small staff, edited its monthly Bulletin, assembled at the Comité probably the major French library on the Resistance, and, more important, has brought together "les témoignages"—the eyewitness accounts of the activities of resisters throughout France, garnered in large measure while memories were still fresh enough to be checked and crosschecked.

In addition to all this, Professor Michel presented the general report to the meeting of the First International Conference on the History of the Resistance Movements held in Belgium in 1958, as well as a similar general report to the meeting of the Second International Conference in Italy in 1961. From 1958 onward, he has served as a member of the International Committee that has steered the activities of these Conferences. Withal, he has never been a popular figure among his colleagues, although he has tended to overwhelm some of them by the sheer amount of his work; nor is he one of America's greatest admirers.

The literature of Resistance in Europe in World War II started almost as soon as the war came to an end. It was largely comprised of volumes of derring-do; these still continue. Then came some good solid historical works, country by country, official and unofficial. Now we are also getting the literature of "thinkers," and in this category Michel does not shine, for he is basically a chronicler gone wrong.

The historians of the Soviet Union and the Bloc have always alleged that the West did not understand the true meaning of the Resistance but utilized it primarily for military and intelligence purposes. The Bloc points out that the purely military aspects of the Resistance were comparatively unimportant, because the occupied countries knew that they would be liberated by the might of the advancing Red Armies with, perhaps, some little additional help from the other Allies. The true activity of the Resistance, the Bloc continued, was a mass uprising of the peoples of the occupied countries, generally led by the Communist parties, with the purpose of liberating the occupied countries not only from the Germans but also from their pre-war oppressive rulers, whom the West hoped to restore to power for the political and economic aggrandizement of Great Britain and the United States.

To the Western historian, it is obvious that the Resistance arose in Europe as a response to the German destruction of their homelands and institutions. The

La Guerre de L'Ombre (Grasset, Paris, 1970).

Resistance which developed represented various philosophies which ranged from the re-establishment of national traditions, and political and religious beliefs, to a search for a new Utopia. Michel stresses the Utopian to the disregard of the former view. His thinking, as represented in *The Shadow War,* has moved quite some distance since 1958, so that today he virtually embraces the Soviet position, although, to be fair to him, he is no Communist and there are elements of criticism of the Soviet view in his book. In private conversations with this reviewer, he has talked of Soviet and Bloc participants at the International Conferences as largely propagandists.

The subtitle of the American edition of Michel's book, "European Resistance," does him no favors. Both the English and the original French editions have the proper subtitle, "Resistance in Europe." This is a major distinction because there was no such thing as a "European Resistance." Michel himself finally makes this point, although it takes him to page 355 to say it. Almost without exception, there was no contact between resistance movements from one country of Europe to another. The major exception was in the field of evasion and escape, where "rat lines" and their contacts did cross some borders. To be sure, there were many similarities in resistance work from country to country, particularly those that were served by the British SOE and later the American OSS, but these were similarities in techniques such as air supply and communications; they did not make a cohesive European Resistance, nor was there any attempt made to produce such a thing. Michel tries in some measure to produce a European Resistance on the basis of the common thoughts for post-war life that the individual resistance movements allegedly had. To some extent this produces a rather misleading text.

Not to his credit, Michel also tries to create a divisiveness between classes of participants, a divisiveness which appears much overdrawn in *The Shadow War.* Perhaps this highlights another one of Michel's weaknesses—a comparative lack of knowledge, for one who has studied the subject so long, of the resistance movements in European countries other than France.

It is not necessary in this review to go into the often retold origins of SOE and OSS. Suffice it to say that London became the capital of the exterior resistance, the major purpose of which, at least in the beginning, was military: the use of the internal resistance for intelligence and sabotage.

In his 1958 report to the First International Conference, Michel writes that "The systematic ignorance of the Americans regarding Resistance would be incomprehensible if it was not caused by their conception of war, which leaves little place for the human factor . . ." He noted there that the OSS had "vast means and little experience." To be sure, OSS had to gather experience, but in *The Shadow War* Michel's anti-Americanism shines through when he states of the Americans and the OSS: "Not even belatedly did they co-operate much with the Resistance. The war the Resistance was waging was the very antithesis of the American concept of industrialized warfare." Yet wasn't it this industry which was able to bring much needed supplies and equipment to the Resistance? Michel continues: "U.S. agents or parachute teams lived divorced from the men of the Resistance whom they had come to help and had no understanding of the ideas, needs or aims of the men alongside whom they were fighting." I believe there are many veterans of OSS who will argue with this statement. It is only with reluctance that Michel admits in *The Shadow War* that by 1944 the United States had become the arsenal of the Resistance, but, he adds, in Central and Eastern Europe "they abandoned the Resistance to its fate."

Michel has an ability to overlook many facts of history. There were both SOE and OSS liaison missions with the Yugoslavs, and once a proper base could be set up in Italy, there were American supplies for the Yugoslav Partisans. The Western Allies would have liked to have come to the support of the Polish Resistance when it rose in Warsaw. It was not a lack of will; it was Stalin's refusal to allow planes attempting to drop supplies to the Warsaw Resistance to land in Russia to refuel that caused the problem. This meant that those planes had to fly the long haul from Italy to Warsaw and return non-stop, greatly reducing their ability to get through with meaningful loads. Michel has perhaps forgotten that it was Stalin himself who rejected General Eisenhower's offer to move from the American stopping point at Pilsen (the agreed on demarcation line) and come to the aid of the Resistance rising in Prague.

Michel does not seem to have any feeling or any great liking for the role of intelligence and its practitioners. That portion of his book which he devotes to what he calls "Movements and Circuits" is a bare 16 pages. In discussing the early exfiltration of personnel from occupied Europe, Michel writes that General de Gaulle "left by air"; that the French and British "helped young Poles to escape"; but when he writes of the escape of the great pre-war and wartime Chief of the Czech Intelligence Service, General Franticek Moravec, Michel writes that the latter "fled with his entire staff." This hardly does a service either to Moravec's memory or to the carefully handled operation by which the British brought Moravec and 10 members of his staff from Prague to London on the day that the Germans marched into Prague. In discussing what the British and Michel call "circuits" and we call "intelligence nets," Michel characterizes them as a "novel organization." The fact of the matter is that they are as old as history. He then notes that "The circuit was a military organization, under strict discipline, its activities severely circumscribed, forbidden to trespass outside the field allotted to it." To characterize these nets as purely a "military organization" is a little wide of the mark. Many of them were composed only of civilians. That the danger of their work might lead them to impose upon themselves a certain amount of internal discipline is a possibility. But those who have read Michael Foot's great volume, *SOE in France*, will remember many incidents where discipline broke down. In another *non sequitur* Michel writes of intelligence activities that "In fact, the entire life and attitudes of an occupied country had to be faithfully recorded—photographic aircraft were used for the same task, but they could only capture the outlines." There is no profundity in his conclusion that it is "extremely difficult to be dogmatic about the part played by the intelligence circuits in the success or failure of military operations." Michel's thoughts are even harder to explain in the one page he devotes to Soviet "circuits." It is Michel's view that the Soviet nets "seem to have differed" from the intelligence nets of the Western Allies on two main counts: "in the first place they followed the traditional pattern of small groups of highly-trained spies and, secondly, the aim of the circuits was conventional military espionage." It is very difficult to see how this description of the Soviet intelligence nets would not be equally applicable to those of the West.

There are a few points which one should make in conclusion about *The Shadow War*. One of them is Michel's habit of thinking in terms of the Western Allies and the Soviet Union. However threadbare the alliance may have been at times, it was an alliance of the three powers, and it has to be thought of in those terms. In the many examples which Michel gives to illustrate his points, the vast majority are taken from the French Resistance, as if there were no other. In its thesis on the resistance in Europe it comes close to being revisionist. It is highly selective both in its thinking and

its examples. Perhaps Michel points this out when he notes: "I make no attempt to give a *complete* account of the action of the Resistance . . . I have confined myself to recounting and comparing certain actions which seemed of significance to my subject." Perhaps Michel would not have fallen off the deep end in his attempt to explain his theories of the politics of the Resistance from the standpoint of the resister, if he had followed the thesis of some other historians that in the deepest sense the Resistance was the moral conscience of the war preserving the beliefs in a free Europe. Lastly, this reviewer would be critical of Michel's bibliography, which is both thin and not very representative.

Walter Pforzheimer

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